AN
EXAMINATION
OF
Dr. *CRAWFORD's* THEORY:

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AN EXAMINATION OF Dr. CRAWFORD'S THEORY OF HEAT AND COMBUSTION.

By WILLIAM MORGAN.

Neceſſarium puto, ut caveamus maxime, ne, inquirentes in rei tam profundé abditæ ingenium, fallamur uſquam. Oportebit igitur nulli ſervire precarió aſſumtæ ſententiæ; niſi velimus per dubia noſmet incertos dare & præcipites.

Boerhaav. de Igne.

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SECTION the FIRST.

A General Account of Dr. CRAWFORD's *Theory of Heat and Combuſtion.*

FIRE, whether conſidered as an *element* or *quality*, ſo univerſally pervades all bodies, its properties and effects are ſo ſurprizing, and its agency ſo ſubtile and powerful, that it is no wonder the reſearches of philoſophers have often been directed to its inveſtigation. How far they have ſucceeded in their purſuits, I ſhall not take upon me to determine; though if we are to judge by the numerous, and I may add very different opinions on this ſubject, I am afraid we ſhall have reaſon to conclude, that notwithſtanding the labours of this enlightened age, it ſtill remains deeply involved in darkneſs and myſtery. It is not however my deſign either to diſplay or to lament the general imperfection of our knowledge in this branch of philoſophy. My attention hath been of late more particularly engaged to the ſubject, by conſidering only *one* of the many ſyſtems to which it has given riſe, and it is to the examination of this alone that I mean to confine myſelf in the preſent treatiſe.

The ſyſtem I allude to was publiſhed ſome time ago by Dr. *Adair Crawford.*—A ſyſtem which like moſt others reſpecting obſcure ſubjects ſeems to have gained no ſmall degree of attention by its *novelty*; and perhaps it may alſo be added, by the ſpecious appearance of ſome of thoſe experiments which have been produced to ſupport it.—Having no predilection for any particular hypotheſis, I have peruſed and conſidered Dr. Crawford's publication with all the candor in my power, and I ſhall now beg leave to lay before the reader the reſult of my enquiries.—Previous however to what I ſhall offer on this ſubejct, it will be neceſſary to give an idea of our author's ſyſtem in general, and of the grounds upon which he has attempted to eſtabliſh it.

Fire, or heat, according to Dr. C. is contained in a great quantity in moſt bodies when at the common temperature of the atmoſphere, but being in this caſe *latent* and producing no effects which render it the object of our ſenſes, he diſtinguiſhes it by the appellation of *abſolute* heat; and if this ſame principle be ſo increaſed that it ſhall exceed the heat of the ſurrounding medium and become obvious both to ſight and touch, or to the latter ſeparately, he denominates it *ſenſible* heat. In other words, fire is ſuppoſed to enter into the compoſition of all bodies. If it exiſts in them only to ſuch a degree as not to render thoſe bodies warmer than the atmoſphere in which they are placed, it is called *abſolute* heat; and if it is accumulated in ſuch quantities as to produce what is meant by heat in the vulgar acceptation of the word, it is called *ſenſible* heat: So that Dr. C's. diſtribution of fire into two claſſes regards only the *quantity*, and not any difference in the *quality* of this principle. Purſuing this idea, he has recourſe to a variety of Experiments to prove that in the ſame temperature of the Air, equal weights of heterogeneous ſubſtances may contain unequal quantities of abſolute heat, and conſequently that the ſame bodies have the power of collecting and retaining the element of fire in greater quantities than others. Theſe different powers he calls the *capacities* of bodies for containing heat.—In determining the comparative quantities of heat in bodies, his Experiments led him to conclude that atmoſpherical and dephlogiſticated air have much more of this principle than Fixed and Phlogiſticated air, and that the quantity of heat in airs of different kinds is in proportion to their purity.—Following the ſame method of making his Experiments on more ſolid bodies, Dr. C. is further led to maintain that Metals have leſs heat than their Calces,* and uniting this to the fact juſt mentioned reſpecting air, he lays down this general law; " That bodies when joined to " phlogiſton contain leſs abſolute heat than " when they are ſeparated from it," and therefore, " that if phlogiſton be added to a body, " a quantity of the abſolute heat of that body " will be extricated; and if phlogiſton be ſe- " parated again, an equal quantity of heat

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* It may not be improper to obſerve here, that thoſe calces are the earthy parts of the metals ſeparated from their phlogiſton.—And that Atmoſpherical and Dephlogiſticated Airs, are ſuppoſed to be more or leſs pure as they are more or leſs free from this principle. An attention to theſe particulars will render the extracts which follow from Dr. C's. Treatiſe more intelligible.

" will be abſorbed." † From all which he infers, " that heat and phlogiſton appear to be " two oppoſite principles in nature." ‡ By the aſſiſtance of this important diſcovery the whole myſtery of Combuſtion is explained. It had hitherto been generally believed (but upon what good grounds I ſhall not pretend to ſay) that the heat which is produced by the inflammation of combuſtible bodies, proceeded from the latent fire contained in thoſe bodies. Dr. C. on the contrary accounts for this phænomenon, by obſerving, that no part of the heat can be derived from the combuſtible body, * (ſo far from it, that the body even *leſſens* the fire by abſorbing the heat;) but that in the proceſs of inflammation the phlogiſton is ſeperated from the inflammable body and combined with the air; the air by this union is converted into fixed and phlogiſticated air, and at the ſame time (in conſequence of the repugnance of phlogiſton to fire) is obliged to part with a very great quantity of its abſolute heat, which when it is extricated ſuddenly burſts forth into flame, and produces that intenſe heat which is always known to accompany the combuſtion of bodies.

† Page 67. ‡ Page 68. * Page 77.

Theſe are the chief of Dr. C's. diſcoveries, or at leaſt they are the particular parts of his ſyſtem which I propoſe to examine. What relates to Animal heat depends ſo much on the repugnancy of phlogiſton to fire, that if theſe two principles can be ſhewn not to be averſe to one another, a theory founded on this mutual averſion, and ſupported likewiſe by ſo few Experiments, will hardly be thought to deſerve much attention.

SECTION the SECOND.

On Dr. CRAWFORD's *Experiments in general.*

THE *manner* in which Dr. Crawford has made his Experiments, and the *principles* upon which they appear to be founded, naturally preſent themſelves as the firſt objects of enquiry. For without being perfectly ſatisfied with the accuracy of the one and the truth of the other, it will not be poſſible to form a ſound judgment of the ſyſtem with which they are connected.

The grand principle on which Dr. C.'s Experiments are founded is the following—Suppoſe equal portions of two heterogenous ſubſtances of any kind and of different temperatures of heat to be mixed together. That which has moſt ſenſible heat will communicate ſo much of it to the colder body, that both ſhall be reduced to the ſame common temperature. Now it is ſaid, that if the differences be taken between the heat of the bodies *before* they were put together and this common temperature of the mixture, thoſe differences will give the comparative quantities of abſolute heat which the bodies contained in their ſeparate ſtate; the differences and quantities above mentioned being always in reciprocal proportion to each other.—Thus; if one pint of Water at 100° be mixed with one pint of Quickſilver at 50°, we are told, that the common temperature of the whole will be nearly 80°,—ſo that the water will have loſt 20° of its heat, and the Quickſilver gained 30°. From whence it is infered, that the comparative quantities of abſolute heat in thoſe bodies are as 30 to 20; that is, one pint of Water has more heat than one pint of Quickſilver in the proportion of 3 to 2; and therefore, by Dr. C's. reaſoning, the ſame quantity of fire, which will require only two parts of water to contain it, will not be contained in leſs than three parts of Quickſilver.

Admitting the Experiment to be as it is ſtated above, yet I cannot ſee upon what grounds this concluſion is founded.—When any particular ſubſtance is added to two ſeparate bodies, and that ſubſtance produces more ſenſible effects in one than in the other; how does it follow that therefore the body in which thoſe effects are greateſt had leaſt of that ſubſtance in its original compoſition? —Suppoſe, for inſtance, we had two ſolutions of any neutral ſalt in water; and being deſirous of knowing which of the two was moſt impregnated, we added equal quantities of the ſame ſalt to thoſe ſolutions, and that in the one a great part of the ſalt was depoſited, or became redundant, while in the other it was almoſt wholly diſſolved. Should we be juſtified under thoſe circumſtances in concluding that the former had *leſs* ſalt in it, than the latter?—Dr. C's. concluſion with regard to heat ſeems to me to be juſt as reaſonable.—Might we not at leaſt with as much appearance of truth argue in a manner directly contrary to what he has done? Suppoſe an equal portion of fire were introduced into two ſeparate bodies of the ſame ſenſible heat, and that the one became much warmer than the other; would it not be as natural to conclude that the former, like the ſolution of the neutral ſalt, by having more of the element of fire in its compoſition had been ſooneſt ſaturated, and therefore that a great quantity of this principle had become ſuperfluous and been converted into ſenſible heat; while the latter by having leſs of the element of fire in its compoſition, had abſorbed a greater portion of this principle, which being converted into abſolute heat had been rendered incapable (according to Dr. C's. hypotheſis) of being diſtinguiſhed by our ſenſes? From hence we ſhould be led to infer the very reverſe of what Dr. C. had done, by ſaying "that the more readily a body is heated the "more fire it contains, and on the contrary "the leſs readily it is heated the leſs fire it "contains." But though this concluſion appears to me the leaſt abſurd of the two, yet I am far from thinking it ſatisfactory, nor can I ſee that Experiments of this kind prove any thing more, than that ſome bodies (as Dr. Franklin obſerves *) are better *conductors* of

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* Allowing common fire as well as electrical to be a fluid capable of permeating other bodies and ſeeking an equilibrium,

fire than others, or are more or leſs capable of being acted upon by heat whatever it is.— The reaſon of the fact that ſome bodies heat others in a greater proportion than they are themſelves cooled may be, that they are ſo conſtituted as to be more or leſs ſuſceptible of that modification which we call heat; and conſequently this fact proves no particular theory of heat. I ſhall not therefore dwell any longer on a ſubject which is ſo much more eaſily controverted than underſtood, but proceeding to the Experiments themſelves endeavour to examine their accuracy, and from thence determine how far they may be depended upon as concluſive in favor of *any* hypotheſis.

In almoſt the whole of Dr. C's Experiments water is employed as an ingredient. The ab-

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brium, I imagine ſome bodies are better fitted by nature to be conductors of that fluid than others.—Thus, if a body which is a good conductor of fire readily receives it into its ſubſtance and conducts it through the whole to all the parts; as metals and waters do, and if two bodies both good conductors, one heated the other in its common ſtate, are brought into contact with each other, the body which has moſt fire readily communicates it to that which had leaſt, and that which had leaſt readily receives it till an equilibrium is produced."

Dr. Franklin's Philoſophical Works, p. 351.—5th Edit.

ſolute heat of this fluid is made to be the ſtandard by which the heat of other bodies is generally determined. The method of doing this, is by mixing water and any ſubſtance together at different temperatures, and noting after a given time the changes produced in their ſenſible heats; theſe changes, as I have already obſerved, being always ſuppoſed to be reciprocally proportional to the quantities of their abſolute heat.—Now it is evident that if the mixture be made in a cold veſſel a part of the heat will be communicated from the warmer body to the veſſel; and if on the contrary it be made in a warm veſſel the cold ſubſtance will acquire its heat, not only from the warmer body with which it is united, but alſo from the veſſel in which the mixture is contained. In order to remove this cauſe of inaccuracy, by determining the portion of heat which is either loſt or communicated by the veſſel in the courſe of any Experiment, Dr. C. proceeds in the following manner. "The air in the room being " 61, one pound of water at 168° was poured " into an earthen veſſel at 68°, the temperature " of the mixture at the end of the firſt minute was 155°—at the end of the ſecond " 150°—at the end of the third 145°.—To " diſcover the heat communicated to the at-

"moſphere

“ moſphere in the firſt minute, ſay as 84 to 89
“ ſo is 94 to a fourth proportional *, which
“ gives 99.5 : From whence it appears that $5\frac{1}{2}$
“ degrees were carried off by the air in the
“ firſt minute. Adding $5\frac{1}{2}$ to 155 we have
“ 160.5 for the true temperature of the water
“ and the veſſel. Subtracting this from 168
“ we have 7.5 for the remainder. The water
“ was therefore cooled by the veſſel 75°, and
“ the veſſel was heated by the water 92°.5 :
“ And ſince the veſſel received this heat from
“ the water, it is manifeſt that the ſame quantity
“ of heat which changes the temperature of
“ one pound of water 7.5 will change the temperature
“ of the veſſel 92.5. And by a parity
“ of reaſoning the ſame heat which raiſes the
“ water one degree will raiſe the veſſel $12\frac{1}{3}$

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* The author in computing the heat which is loſt in the firſt minute adopts a rule which is ſaid to be given by Sir *Iſaac Newton*, who conſidering the heat of a body as the exceſs whereby it is warmer than the ſurrounding medium, ſuppoſed that the quantities of heat loſt in ſmall portions of time would always be proportional to the heats remaining, and therefore if the times were taken in arithmetic progreſſion the decrements of heat would be in geometric progreſſion.——I would further obſerve that the numbers 94, 89 and 84 are found by ſubtracting 61, the temperature of the atmoſphere, from 155, 150 and 145, the ſeveral degrees of heat at the end of the 1ſt, 2d, and 3d minutes.

“ degrees. If therefore in any Experiment we “ find that the veſſel has received 12⅓ degrees “ of heat from one pound of water, we may be “ ſure that the ſeparation of this heat has cooled “ the water one degree.” Were this rule of Dr. C’s. ſtrictly right, it would give ſimilar reſults in all caſes; that is, were the ſenſible heat of the room, the veſſel, or the water varied ever ſo much, we ſhould ſtill find the capacities of the two latter for retaining heat exactly in the ſame proportion to each other.—I have made a great variety of Experiments with a view to determine this, but their reſults have always been ſo different, that I am convinced no concluſions derived from ſuch Experiments can be depended upon. The following out of the many inſtances which I could produce, will, I imagine, be ſufficient to give an idea of the accuracy of Dr. C’s. rule.

EXPERIMENT. I.

Air in the room being 61, half a pound of water at 185 was poured into an earthen veſſel at 60. The temperature was at the end of

	Surface	Bottom	Medium
the firſt minute	169½	166½	168
the ſecond minute	162	160	161
the third minute	156	155	155½

By following the ſame ſteps with Dr. C. for diſcovering the heat which was loſt in the firſt minute we have 94.5 to 100 as 107 to 113.2. Therefore 6.2 being added to 168 gives 174.2 for the temperature of the water and veſſel: From whence it may be eaſily ſeen that the capacities of the veſſel and water, for retaining heat are to each other in the ratio of 1 to 10½ *.

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* The Experiments in this treatiſe have been almoſt univerſally repeated twice, and ſome of them much oftener. It is alſo neceſſary to obſerve that whenever two or more Thermometers are employed in the ſame Experiment, care has been taken that thoſe Thermometers are graduated in ſuch a manner as to agree exactly with each other. It is very difficult to find a tube ſo perfectly cylindrical as that the expanſions of the mercury may be truly aſcertained by a ſcale of equal degrees. This muſt be the cauſe of inaccuracy in Experiments of this kind: To which may be added another cauſe ariſing from the difference in the ſize of the bulbs, eſpecially when the bodies cool very faſt. In order to obviate thoſe difficulties, two thermometers are placed at the ſame inſtant in a veſſel of hot water, and the ſituation of the mercury as it falls in both is marked every 4 or 5 degrees, by the aſſiſtance of another very good Thermometer placed in the ſame circumſtances; ſo that after repeated trials in veſſels where the water cools differently, ſuch ſcales are obtained as to make the two Thermometers coincide exactly with each other.—This is a work of much care and trouble, but in Experiments which depend entirely upon the accuracy with which they are made, no care or trouble ought to be avoided.

EXPERIMENT II.

Air in the room being 61, half a pound of water at 154 was poured into the ſame veſſel at 61. The temperature of the water was at the end of

	Surface	Bottom	Medium.
the firſt minute	144	142	143
the ſecond minute	140½	139	139¾
the third minute	137	136	136½

Hence we have 75.5 to 78.75 as 82 to 85.5, and conſequently the ratio in this caſe is as 1 to 11.4.

EXPERIMENT III.

Air in the room being 62, half a pound of water at 106 was poured into the ſame veſſel at 63. The temperature of the water was at the end of

	Surface	Bottom	Medium
the firſt minute	102½	101½	102
the ſecond minute	101	100	100½
the third minute	99¾	99¼	99½

Therefore 37.5 to 38.5 as 40 to 41.06: From whence the ratio of the veſſel's capacity for retaining heat to that of the water may be found as 1 to 13.6

It may be obſerved from theſe Experiments that the capacity of the ſame veſſel for containing heat when compared with that of water is *one third* greater in ſome circumſtances than in others :—And the like uncertainty takes place in the reſults of Experiments upon other bodies when they are made in this manner. I ſhall beg leave to give an inſtance of this kind with reſpect to the comparative quantity of heat in oats, which is one of the firſt ſubjects of Dr. C's. enquiry.

EXPERIMENT IV.

Air in the room being 65, three-quarters of a pound of oats at 71 were mixed with an equal weight of water at 116. The temperature of the mixture was at the end of

	Surface	Bottom	Medium
the firſt minute	101	100	100½
the ſecond minute	99	98	98½
the third minute	98	96½	97¼

Hence we have 32.25 to 33.5 as 35.5 to 36.9; and conſequently the heat carried off by the air in the firſt minute was 1.4. This being added to 100.5 gives 101.9 for the true temperature of the mixture. The capacity of the veſſel for retaining heat to that of water was found by Dr. C's. rule to be in the ratio of 1 to 8; and ſince the veſſel in the preſent Experiment was raiſed from 71 to 100.5*, the difference (or 29.5) being divided by 8 gives 3.7 for the heat which was ſeparated from the water by the veſſel—Subtracting 101 (the true temperature of the mixture) from 116, and 3.7 again from the remainder, we have 10.4 for the number of degrees which the water was cooled by the oats alone†. And ſince the oats were raiſed from 71 to 101.9 or 30.9 degrees, it follows, that the ſame quantity of heat which will change the temperature of the water 10.4 will change that of oats 30.9, and therefore the abſolute heat of water is to that of oats as 30 9 to 10.4, or nearly as 2.9 to 1.

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* I have exactly followed Dr. C.'s method in making this Experiment, but I can ſee no good reaſon for deducting the heat of the veſſel from the temperature of the mixture *at the end of the firſt minute*, rather than from the *fourth proportional*, which is ſuppoſed to give the true temperature of the mixture *at the beginning of the firſt minute.*

† It does not appear that the oats in this Experiment were really heated to the degree ſuppoſed by Dr. C.—The Thermometer might only ſhew for ſeveral minutes the joint effects of the hot particles of water and the colder particles of oats in contact with it. For this reaſon Sir Iſaac Newton's rule ſeems to be improperly applied; for the heat carried off after the firſt minute may not be carried off by the air only, but by the air and the cold ſubſtance jointly.

EXPERIMENT V.

Air in the room being 67, three-quarters of a pound of oats at 70, were mixed with an equal weight of water at 164.—The temperature of the mixture was at the end of

	Surface	Bottom	Medium
the firſt minute	131½	125	128¼
the ſecond minute	127½	123½	125½
the third minute	125½	122½	124

From whence it may be found, by proceeding in the ſame manner as in the foregoing Experiment, that the abſolute heat of water is to that of oats in the ratio of 2¼ to 1 *.

Here again we may obſerve, that the ratio in one experiment is nearly *one third* greater than in the other.—But theſe inaccuracies are not confined to this mode only of making Dr. C.'s Experiments. For if inſtead of heating the *water* the *ſolid body* be heated, as he has done in many inſtances, we ſhall find the reſults to be ſtill as uncertain, and that the Experiments when they are made in this way, not only differ from thoſe which are made in a contrary way, but that, like the five preceding Experiments, they differ alſo from each other.—Various inſtances of this kind might be given. Oats, barley, metals, calces of metals, in ſhort every ſubſtance that I have tried are ſo many proofs of this fact. But I wiſh to be conciſe, and ſhall therefore content myſelf with relating a few Experiments on Quickſilver, which will be quite ſufficient for the preſent purpoſe.

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* The ratio in Dr. C.'s Experiment on oats, made nearly under the ſame circumſtances, is as 2½ to 1.

EXPERIMENT VI.

Air in the room being 58, one pound and a half of mercury at 61 was mixed with an equal weight of water at 159. The temperature was at the end of

	Surface	Bottom	Medium
the firſt minute	145	143½	144¼
the ſecond minute	142½	140½	141½
the third minute	140½	138	139¼

The capacity of the veſſel in which the mixture was made, was to that of water as 1 to 11½. From whence it may be found by proceeding in the ſame manner as is deſcribed in Experiment the 4th, that the abſolute heats of mercury and water are to each other in the ratio of 1 to 15.9.

EXPERIMENT. VII.

Air in the room being 58, one pound and a half of mercury at 58, and an equal weight of water at 113½ were mixed together in the ſame veſſel as in the foregoing Experiment. The temperature of the mixture was at the end of

	Surface	Bottom	Medium
the firſt minute	107½	106½	107
the ſecond minute	106¾	105¼	106
the third minute	105	104½	105¼

The abſolute heat therefore of the mercury is to that of the water in the ratio of 1 to 34.6.

EXPERIMENT VIII.

Air in the room being 61, one pound and a half of mercury at 181 was mixed with an equal weight of water at 65. The mixture was at the end of the firſt and ſecond minutes 68¼. The mercury therefore was cooled 112° and the water heated 3°½. But the capacity of the veſſel for retaining heat being to that of water in the ratio of 1 to 11½, it follows according to Dr. C. that the heat abſorbed by the veſſel was equal to $\frac{1}{11.5}$ of lb 1½, or to 1½oz. of water. Since therefore the mercury heated 18 ounces * of water together with the veſſel which was equal to one ounce and a half, 3°¼, it would con-

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* It ſhould be obſerved that *Trey* weight is conſtantly uſed in all thoſe Experiments.

ſequently have heated 19 ounces and a half 3°¼; and hence the abſolute heat of mercury is to that of water in the compound ratio of 3¼ to 112¾ and of 19½ to 18, or as 1 to 32.8.

EXPERIMENT IX.

Air in the room being 61, one pound and a half of mercury at 115° was mixed with one one pound and a half of water at 60°. The mixture at the end of the firſt and ſecond minutes was at the ſurface and bottom 62°¼. From whence it follows that the abſolute heat of mercury to that of water is in the ratio of 1 to 21.6.

The reſults of thoſe Experiments are ſo very ſtriking, that it is needleſs to make any comment upon them. I cannot, however, diſmiſs the ſubject without obſerving, that our author has given us a third rule for determining the heat of bodies; by which he has been enabled to aſcertain the inconceivable heat of the air we breathe, and from thence to lay the foundation for one of his principal diſcoveries. This rule is ſaid to have been pointed out by Dr. Irvine of Glaſgow, and is ſimply no more than "that the comparative quantities of abſo-"lute heat in two bodies are reciprocally as the "changes produced in their ſenſible heats

" multiplied into the quantities of matter they " contain." For inſtance, if equal bulks of mercury and water be mixed together at different temperatures, the changes produced in their ſenſible heats will (as we have already obſerved) be as 3 to 2; that is, if one pint of mercury at 50 be mixed with one pint of water at 100, the former will be heated 30°; and the latter cooled only 20°. But the ſpecific gravity of one pint of mercury being 14 times greater than an equal bulk of water, it follows according to this rule that the abſolute heats of mercury and water are *reciprocally* as 3+14 to 2, or *directly* as 1 to 21. Here again we find a new proportion between the heats of theſe two bodies; nor is it poſſible that the truth of this Experiment ſhould be controverted, unleſs the general rule be wrong, which I fancy Dr. C. will hardly allow, who has founded ſo great a part of his ſyſtem upon it.—Mr. *Kirwan*, who appears to have warmly adopted Dr. C's. theory, makes the heat of mercury to that of water in the ratio of 1 to 30 nearly*. I do not ſee that any thing can be urged in favour of ſuch contrarieties. It ſeems abſolutely neceſſary that the comparative quantities of heat in bodies ſhould be found preciſely the ſame by every method of diverſifying the Experiment; and until this be done, I believe it will be acknowledged, that the truth of Dr. C's ſyſtem is *more* than precarious.

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* See Mr. Magellan's Eſſai ſur la Nouvelle Theorie de la Feu Elementaire, &c. page 177.

Beſides the difference in the *Reſults* of Dr. C's Experiments, there are inaccuracies in the *Experiments themſelves* which deſerve to be conſidered. The application of Sir *Iſaac Newton's* rule for determining the heat loſt the firſt minute is in ſome caſes very erroneous. For if this rule were right, the heat of the mixture, as it is ſet down for the firſt minute in Dr. C's Experiments, would always be a fourth proportional to the heat in the fourth, third and ſecond minutes. Now we have but few inſtances where the degrees of heat are given during four minutes, and where at the ſame time the above-mentioned rule has been applied. In moſt of theſe however the inaccuracy is very manifeſt; in ſome of them it is particularly ſo. Thus, in Experiment 2, page 25*, had the heat at the end of the firſt minute been taken as a fourth proportional to the heat in the three ſubſequent minutes it would have been $126\frac{1}{3}$, or $1\frac{1}{3}$ *more* than the true heat, and in Experiment 3, page 26†, it would have been only 115°, or 1° *leſs* than the true heat. It is evident therefore that this rule is not to be depended upon in all caſes; and that by the indiſcriminate uſe of it in Experiments of this kind, which require the utmoſt nicety, we ſhall be often led into conſiderable errors.

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* Dr. C.'s treatiſe. † Ibid.

Another cauſe of inaccuracy in Dr. C.'s Experiments proceeds from his method of aſcertaining the heat of the mixture at the bottom of the veſſel. If we examine thoſe inſtances where the heated metallic calces are mixed with cold water, we ſhall conſtantly find the mixture hotter at the bottom than the top of the veſſel. Now it is very well known that heated water is always warmeſt at the ſurface. Whatever heat therefore it receives from the calx will fly up in the greateſt proportion to the top of the mixture. But we frequently find the very reverſe of all this in Dr. C.'s Experiments: Nor ſhall we be ſurpriſed at it, if we only conſider the manner in which he determines the heat at the bottom. That the metallic calces are bad conductors of fire is a fact which has long been obſerved, and that conſequently when they are mixed in a heated ſtate with a cold fluid they will be a conſiderable time in imparting their heat to that fluid. The reaſon therefore of the mixture's being hotteſt at the bottom in all Dr. C's Experiments on the calces of metals is very obvious; for by plunging the thermometer into the midſt of the calx, it does not denote the heat which has been communicated to the water, but the heat which the calx ſtill retains; and it is evident that the true additional heat of the water can only be aſcertained at the ſurface; or at leaſt where the calx is not in contact with the bulb of the thermometer. A ſtrong proof of this may be ſeen in Dr. C.'s Experiments on the metals themſelves; for in thoſe caſes, where the heated body is in a more compact form and leſs in contact with the thermometer, the water is conſtantly of the ſame heat in every part. Nay even in the Experiments on the metallic calces, if we only uſe a ſmall ſtrainer ſo as to keep the calx from lying immediately upon the thermometer, the water will be as warm, and in ſome inſtances warmer, at the ſurface than at the bottom. It may perhaps be objected that the water under thoſe circumſtances, by being confined within the ſtrainer, is not in a ſtate to imbibe the heat from the calx. This however is not the caſe; for the ſtrainer may be ſo conſtructed that it may be moved together with the thermometer thro' every part of the mixture and freſh portions of water be continually paſſing through it; and yet the ſame reſult will take place. But it is hardly neceſſary to uſe many arguments on this ſubject. If we carefully examine Dr. C's Experiments, I am afraid we ſhall ſee reaſon, not only to doubt the *method* he has adopted in making them, but the *accuracy* with which this method has been purſued: For in ſome Experiments, we find the heat at the bottom increaſing for ſeveral minutes while that at the ſurface is decreaſing; in others on ſimilar bodies and under the ſame circumſtances the very contrary takes place*.—Nay in one Experiment the heat firſt decreaſes at the ſurface for two minutes, it then increaſes for the ſame time, it afterwards decreaſes for a minute, and laſtly becomes ſtationary †. It it not very difficult to aſſign the cauſe of thoſe incongruities. The whole depends upon keeping the thermometer ſteady after properly mixing the two bodies together. If this be obſerved, the mixture goes on to cool uniformly in every part, but if the thermometer be moved, or the mixture be otherwiſe agitated, a freſh ſurface of the heated body is expoſed to the water, and of courſe the uniformity is diſturbed.

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* Compare Experiment 2. page 25, and Experiment 4. page 27, with Experiment 3. page 26. Compare alſo Experiments 2. and 5. with Experiments 3. and 8. Propoſition. 2.

† See Experiment 6. Prop. 1.

The method of taking the arithmetical mean between the heat at the ſurface and that at the bottom of the veſſel in all mixtures indiſcriminately is another ſource of error. The portions of water, for inſtance, in contact with a bulky vegetable and the ſolid calx of a metal are ſo very different in the Experiments on thoſe bodies, and of conſequence the heat is ſo differently diffuſed throughout the mixture, that in neither caſe will Dr. C.'s mean give the real degree of heat which the water has received or communicated. Nay if hot water be poured without any additional ſubſtance into a cold veſſel, we ſhall never find the heat in the middle to be equal to ſuch a mean, but always greater than it. —Several other facts might be produced to invalidate this rule, and many further objections might be made to Dr. C.'s Experiments in general; but I am apprehenſive of becoming tedious, and ſhall therefore diſmiſs the ſubject, believing that the concluſions deduced from thoſe Experiments muſt be rendered *precarious* at leaſt by the inaccuracies which I have already pointed out.

SECTION the THIRD.

An Examination of the Propoſitions upon which DR. CRAWFORD'S *Theory is founded.*

DR. Crawford's whole ſyſtem is founded on the demonſtration of the three following Propoſitions. Firſt, "That atmoſpherical air "contains a greater quantity of abſolute heat "than the air which is expired from the lungs "of animals, and that the quantity of abſolute "heat in any kind of air is very nearly in pro-"portion to its purity, or to the power of ſup-"porting animal life." Secondly, "That the "blood which paſſes from the lungs to the "heart by the pulmonary vein, contains more "abſolute heat than that which paſſes from "the heart to the lungs by the pulmonary "artery." Thirdly, "That the capacities of "bodies for containing heat are diminiſhed by "the addition of phlogiſton, and increaſed by "the ſeparation of this principle."

In attempting to prove the truth of the firſt propoſition, Dr. C. begins with determining the quantity of abſolute heat in common or atmoſpherical air; and, in order to this, adopts Dr. Irvine's rule * in making his Experiments. Some of theſe I ſhall beg leave to tranſcribe—not with a view of diſputing the accuracy of the *Rule*, or the principles upon which it depends, but of examining the *facts* themſelves as they are ſtated in thoſe Experiments.

EXPERIMENT, I. †

" Air in the room being 52, a bladder con-
" taining one pint of atmoſpherical air at 102
" was immerſed in one pint of water at 52,
" the heat of the water at the end of

	Surface	Bottom
the firſt minute was	53	52¼
the ſecond minute	53	52½
the third minute	53¼	53
the fourth minute	53¼	53¼

" In this Experiment ‡ the heat commu-
" nicated to the water by the air and bladder
" which contained it was one degree and a
" quarter. Of this quantity of heat the por-
" tion yielded by the bladder was determined
" in the following manner."

EXPERIMENT II.

" One pound of water was taken at 52.

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* See the laſt Section. † Dr. C's Treatiſe, page 34.
‡ Ibid. page 36.

“ The bladder having been dried and freed from air was raiſed to 102, and being immerſed in the water, the heat of the water was at the end of

	Surface	Bottom
the firſt minute	$52\frac{1}{4}$	52
the ſecond minute	52_{4}	$52\frac{1}{4}$

“ One quarter of a degree muſt be allowed therefore for the heat imparted by the bladder, and hence it follows, that one degree was communicated by the air in the firſt Experiment.”

Being poſſeſſed of this *datum* and applying Dr. Irvine’s general rule, our Author finds that the abſolute heat of air is to that of water in the ratio of 18.6 to one nearly.—Purſuing the ſame courſe in making his Experiments on phlogiſticated and fixed airs he finds that no more heat is imparted to the water from thoſe airs and the bladder in which they are contained, than when the ſame bladder is raiſed to an equal temperature and immerſed in the water without air. But with regard to pure dephlogiſticated air the caſe is ſaid to be very different. Here the quantity of heat communicated to the water is found to be ſo conſiderable *, that the abſolute heat of the one is to that of the other in the ratio of 87 to one.

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* Ibid. page 51.

Admitting Dr. C's principles to be true, and at the ſame time that he has not been deceived in making his Experiments, theſe facts appear to be deciſive. But I muſt confeſs, that though I have repeated thoſe Experiments in all the variety of methods I could think of, I have never yet been able to diſtinguiſh the leaſt difference between any kinds of air. It is a pity that Dr. C. had not been more particular with reſpect to the manner in which he made thoſe Experiments. This would have led us to diſcover where the inaccuracy lay. At preſent we are left to conjecture; though perhaps the following Experiments may aſſiſt a little in directing us.

EXPERIMENT I.

Air in the room being 64, three quarters of a pint of common air was introduced into a bladder (Fig. 1ſt.) (*d*) in which a Thermometer (*a*) had been previouſly fixed, and the whole was covered over with hot ſand till the mercury roſe to 112°. The bladder was then taken out, and with the utmoſt expedition immerſed in one pint of water at 62°¾. The thermometers in the water gradually roſe, till at the end of three minutes both ſtood at 63½.

EXPERIMENT II.

The ſame bladder being dried and again filled with the ſame quantity of common air, was covered over with ſand till the thermometer roſe to 112°. The heated air was then preſſed out through the perforation in the cork (*b*), which being inſtantly cloſed with the ſmall cork (*c*), the bladder thus emptied was immerſed in one pint of water as before at 62°¾, and both the thermometers ſtood at the end of three minutes *above* 63¼.

Inflammable air, fixed air, and phlogiſticated air were tried in the ſame manner, and the reſults were exactly ſimilar: The empty bladder in all thoſe Experiments raiſing the water within leſs than a quarter of a degree as high as the ſame bladder diſtended with thoſe different kinds of air.——It may be obſerved that in heating the bladder in the ſecond Experiment it was previouſly filled with air.—This ſtep I take to be abſolutely neceſſary; for it is not poſſible in any other way to aſcertain whether the bladder has become of the ſame temperature as it had been in the foregoing Experiment.

ment—In this caſe all circumſtances are ſtrictly the ſame, the bladder is uniformly heated, its temperature is obtained with accuracy, and what is of moſt conſequence, the ſame ſurface is expoſed to the water as when it is diſtended with air.—Were I to hazard a conjecture I ſhould imagine that an inattention to this particular has been one chief cauſe of Dr. C's error:—And I am the rather inclined to this opinion from obſerving, that if the bladder be ſhrivelled up, though raiſed to 112°, it will not communicate above one quarter of a degree of heat to the water, which is nearly in the ſame proportion as Dr. C. has found it under ſimilar circumſtances.

I have not only varied thoſe Experiments by taking the air and water at different temperatures, but inſtead of covering over the bladder with hot ſand I have immerſed it in hot water till the thermometer, incloſed within, has been raiſed to a given height; and in this manner I have aſcertained the difference between the heat communicated by the air and bladder together, and the heat communicated by the bladder alone; but I have never found an inſtance which would juſtify me in ſaying, that one kind of air has a greater power of heating water than another; or according to Dr. C.'s principles, that one kind of air contains more abſolute heat than another.

It will be proper to add here, that in lieu of a bladder, as is repreſented in Fig. 1, I have heated the air in a Florence flaſk and immerſed it in water, but with the ſame ſucceſs;—having never been able to diſcover the leaſt difference in the heat communicated by the pureſt dephlogiſticated air, and by the moſt noxious fixed air.——Conſidering the great number of trials I have made, and the ſtrict care I have taken that every minute circumſtance ſhould be the ſame, I am not diſpoſed to believe that I can poſſibly have been miſtaken.—Still, however, I do not think it the leſs neceſſary to inſert the following method which I adopted, in order to be further ſatisfied on this ſubject.

EXPERIMENT III.*

To a thick deal board (*a*) about five feet long and one and a half wide were fixed two ſtands (*k*) and (*e*.) On the former (*k*) was placed a pail (*d*) of hot water at 116°, and on the latter (*e*) a tin veſſel (*f*) with one pint of cold water at 64°. In the pail (*d*) was immerſed a bladder (*b*) (to which a braſs ſtop-cock (*i*) had been previouſly fitted) containing a quantity of common air.—In the ſide and near the bottom of the tin veſſel (*f*) was fixed a hollow tube (*e*) with one end projecting half an inch through a hole in the deal board (*a*), and with the other end projecting on the inſide of the tin veſſel ſo as to have the neck of a flaccid bladder (*g*) tied round it. By this means a communication was formed between the two bladders (*b*) and (*g*), while at the ſame time the hot water in the pail was prevented from affecting the temperature of the water in the tin veſſel (*f*) by the board (*a*) which intercepted them. The flaccid bladder (*g*) which lay floating in the cold water, would contain, when full, one pint of air.—In this ſame water, and ſo as not to touch the ſide of the tin veſſel, a thermometer (*h*) was alſo ſuſpended. Things being thus prepared, and having kept the bladder (*b*) and the braſs ſtop-cock (*i*) together with both my hands under the water in the pail, till the air in the bladder had come within two degrees of the ſame heat with this water, I gently lifted up the mouth of the braſs cock, and, carefully obſerving in the mean while that the bladder was entirely immerſed, took out a ſmall cork which had been inſerted with a view of preventing any hot water from getting into the cock and conſequently being forced into the bladder (*g*). I then fixed this cock into that part of the hollow tube (*c*) which projected through the deal board, and with my hand under the water preſſing the bladder (*h*), the heated air which it contained inſtantly paſſed into the other bladder (*g*), which being now expanded, elevated the water in the tin veſſel (*f*) ſo as to be totally covered by it. Upon which I turned the braſs cock (*i*) to exclude any communication with the remaining air in the bladder (*h*), and let the whole continue in this ſtate for three minutes;—but during all that time the water in the tin veſſel (*f*) was not raiſed *one tenth* of a degree.

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* Fig. 2d.

In the ſame manner I repeatedly tried phlogiſticated air, fixed air, inflammable air, and even dephlogiſticated air, but never could obſerve the leaſt difference. In all of them the heat communicated to the cold water was ſo trifling, that notwithſtanding the ſcale of my thermometer was very large, I could hardly perceive any alteration in the mercury.—That the bladder (*g*) did abſorb a part of the heat will be readily admitted; but then, allowing

Dr. C. to be right, it could have abſorbed no more than it had communicated to the water when he made his Experiments, and conſequently the greateſt part muſt have paſſed thro' the pores of the bladder into the water;—where, according to his principles, it ought to have produced in the caſe of common and dephlogiſticated air very ſenſible effects.

Since theſe ſpecies of airs do not appear to be more remarkable in their effects than others, and conſidering likewiſe that the effects produced by every kind of air are almoſt imperceptible; may it not be fairly concluded from Dr. C's own principles, that either all airs contain the ſame quantity of abſolute heat; or if there be any difference, that no Experiments have hitherto been able to aſcertain it; and that in whatever inſtances the reſults have varied under ſimilar circumſtances they moſt probably have been owing to the inaccuracy with which thoſe Experiments were made?

I obſerved (page 30) that Dr. C. could diſcover no heat to be communicated to water from fixed and phlogiſticated airs, and conſequently that he only inferred in general that they contained leſs abſolute heat than common air, without being able to aſcertain in what proportion. He has however attempted to remove this difficulty with reſpect to fixed air by determining the comparative quantities of heat in chalk and quicklime, and from thence the comparative quantities of heat in fixed air and water.—Though what has been already ſaid on this ſubject may have rendered any further remarks unneceſſary; yet, ſince a great number of our author's ſubſequent concluſions are derived from this inveſtigation, it will not perhaps be improper to give ſome account of it.

"Chalk and limeſtone, Dr. C. obſerves, "contain more than one third of their weight "of fixed air *, and when they are deprived "of this fluid are converted into quicklime." In order, therefore to aſcertain with accuracy the abſolute heat of fixed air, the firſt ſtep that is taken, is to compare the heat of crude calcareous earth with that of quicklime.

EXPERIMENT I. † "One pound of chalk at 58, was mixed with one pound of water at 158; the temperature of the mixture at the end of the firſt minute was 135, and at the end of the ſecond minute 134.—Adding one degree for the heat carried off by the

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* Page 42.

† Page 43.

the air in the firſt minute we have 136 for the temperature of the mixture. The chalk was raiſed 78°. and the water cooled by the chalk and veſſel together 22°. It was cooled by the veſſel two degrees nearly; it was therefore cooled 20 degrees by the chalk, and hence the heat of water is to that of chalk as 78 to 20, or as 3.9 to 1."

EXPERIMENT II. * "Air 64, half a pound of powdered chalk at 64 was mixed with an equal weight of powdered chalk at 164. The mixture at the end of one minute was 116° — two minutes 114°, — three minutes 112° — five minutes 111° — eight minutes 110°.—The veſſel in which this Experiment was made was heated to 114 previous to the mixture; hence it appears that equal quantities of chalk at different temperatures being mixed together, the temperature of the mixture at the end of two minutes was half the exceſs of the hotter above the colder."——

EXPERIMENT III. † "One pound of quick-lime at 61 was mixed with one pound of chalk at 161; the mixture at the end of

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* Page 44, † Page 45.

one minute was 110—five minutes 110,—ſix minutes 111,—eight minutes 112,—and twelve minutes 110. The veſſel was heated to 111° previous to the mixture. Taking 112 for the common temperature, we have 49 for the heat ſeparated from the chalk, and 51 for that gained by the quicklime, and hence the abſolute heat of chalk is that of quicklime as 51 to 49, or nearly as 25 to 24."

Dr. C. next proceeds to prove by the converſe of one Experiment, and a long train of deductions; that the quantity of heat contained in the earth and air when ſeparated, is not greater than the heat which they contained previous to their ſeparation; or in other words, that the ſum of the heats of the fixed air and quicklime is juſt equal to the heat of the chalk before it had been calcined. From theſe *data* the abſolute heat of fixed air compared with that of chalk is determined in the following manner.—The heats of equal quantites of chalk and quicklime have been ſhewn to be as 25 to 24: conſequently, the heat of any given quantity of chalk to *two thirds* of that quantity of quicklime will be as 25 to 16. Now ſince this is the proportion of quicklime in chalk, and the remaining *third* is fixed air, (which together with the quicklime is proved to contain as much heat as the chalk previous to its calcination) it follows, that the difference between 25 and 16 (or 9) expreſſes the quantity of heat in fixed air, and, therefore, taking equal quantities of chalk and fixed air, the heat of the fixed air will be to that of chalk as (9×3 or) 27 to 25. But by the firſt Experiment it appears that the heat of chalk is to that of water as 1 to 3.9. The heat of fixed air therefore is to that of water as 27 to 25×3.9 or nearly as 1 to 3.6.

With regard to the firſt Experiment in this very complicated inveſtigation, it may be obſerved that it is defective in ſeveral reſpects. The heat of the air in the room is omitted, the different temperatures at the ſurface and bottom are not ſpecified, the heat of the mixture is only given for two minutes, during which time the chalk cannot poſſibly have acquired its greateſt heat, for Dr. C. repreſents this proceſs to be a very ſlow one*; in ſhort the whole Experiment bears ſo much the appearance of inaccuracy, that nothing can be concluded from it with any tolerable preciſion.

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* Page 45.

The deſign of the ſecond Experiment, we are told, is to determine the accuracy of the third; that is, becauſe the temperature of equal portions of hot and cold chalk mixed together was half the exceſs of the hotter above the colder at the end of *two* minutes, the temperature of cold quicklime and hot chalk mixed in the ſame manner may be truly aſcertained at the end of *eight* minutes. Now if in the latter of theſe two Experiments the heat be taken when it is greateſt, I ſee no good reaſon for declining the ſame rule in the former; unleſs it be to avoid the abſurdity which muſt enſue, by taking the temperature of the mixture at any other period than at the end of the ſecond minute; for in this caſe it would follow, *that chalk contains more heat than itſelf* †. In mixing two ſolid ſubſtances together at different temperatures, it is hardly poſſible to blend them ſo intimately as that the heat in the courſe of a few minutes ſhall be equally diffuſed through the whole; and this being the caſe, it is evident that the reſult of an Experiment will depend very much on that part of the mixture in which the thermometer is fixed. The two preceding Experiments are ſtriking proofs of this: For in one, by happening to place the thermometer in the hotteſt part of the mixture, the mercury is higheſt at the end of the firſt minute, and gradually falls every ſucceeding minute, while the hotter particles are diffuſing their heat to the colder ones ſo as to make the whole of the ſame temperature;—in the other, where the thermometer happens to have been fixed in the coldeſt part of the mixture, the very reverſe of all this takes place, and the mercury riſes (though irregularly) for the ſpace of eight minutes.—We have ſeen, however, that Dr. C. finds no difficulty in reconciling theſe two Experiments to each other, and in deducing concluſions from them to ſupport the truth of his ſyſtem. But it is a pity he did not endeavour to be more exact in obſerving that every minute circumſtance in both Experiments were ſtrictly the ſame: For without this precaution, it is impoſſible to deduce any concluſions with the leaſt certainty or ſatisfaction.

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† Had the heat of the mixture in the third Experiment been taken as it was at the end of the firſt five minutes, the abſolute heat of quicklime inſtead of being *leſs* would have been *greater* than that of chalk in the proportion of 51 to 49, and the heat of fixed air, as deduced from hence by Dr. C, would have been different.

Beſides the inaccuracies of theſe Experiments it is alſo evident that nothing can be more precarious than the method which Dr. C. has taken, of determining the heat of fixed air *when it is a conſtituent part of the calcareous earth.* In this combined form it becomes ſo many hundred times * denſer than in a ſeparate ſtate, that it ſeems hardly poſſible to aſcertain its real properties by conſidering it in a form ſo very different from its natural one. How ſadly ſhould we be miſled, were we to judge of the effects of the marine acid from a ſolution of common ſalt in water; or of the corroſive nature of quicklime from the harmleſs powder of marble?—But it is unneceſſary to expatiate on the amazing changes which bodies undergo by chemical combinations. This is a ſubject with which Dr. C. is undoubtedly well acquainted; and I am ſurprized that he ſhould not have attended more to it in the preſent inveſtigation.

It is not my deſign to enter minutely into Dr. C.'s theory of *animal heat.* I cannot however, paſs over the Experiment ‡ by which he proves the truth of his ſecond Propoſition without obſerving, that it appears rather precipitate to found a ſyſtem upon *one* fact, and that fact ſupported only by *one* Experiment, which alſo is far from being the leaſt exceptionable in his treatiſe.—Indeed if we attend here to thoſe ſources of inaccuracy, which I have already pointed out in Dr. C.'s Experiments in general, we ſhall not perhaps be at a loſs to account for the very ſmall variation * in the reſults of the two parts of this Experiment, without having recourſe to the different quantities of heat in arterial and venous blood. — It is a little remarkable that our author in a former Experiment (page 29) for aſcertaining the quantities of heat in a mixture of theſe two ſorts of blood makes it to be nearly as great as the quantity which has been found in the preſent Experiment to be contained in arterial blood alone; the abſolute heat of the latter being to that of the former in the ratio of 97.6 to 97.08, or only of 2.3 to 2.29.—The difference in theſe quantities is vaſtly too inconſiderable even on Dr. C.'s own ſuppoſition. Nay, if it were admitted that the proportion of venous blood was only *one third* of the mixture, it would ſtill follow from that Experiment that it contained within $\frac{1}{70}$ part as much abſolute heat as is contained in arterial blood.—A difference (conſidering the nature of the Experiment) ſo very trifling that it might ſafely be rejected.—Were we therefore to reaſon from this fact, (which perhaps might be done with as much propriety as from the preſent Experiment) we ſhould be led to conclude that the quantities of abſolute heat in venous and arterial blood are *equal*, and by this means overturn the whole ſyſtem of animal heat.

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* The Hon. Mr. Cavendiſh makes the ſpecific gravity of fixed air to be to that of water as 1 to 563, (*a*) and Mr. Cotes makes the ſpecific gravity of water to be to that of limeſtone as 1 to $2\frac{7}{10}$ (*b*). It follows therefore that the ſpecific gravity of fixed air and limeſtone are to each other as 1 to 1520.

(*a*) Phil. Tranſactions, vol. 56, N. 19. (*b*) Cotes's Hydroſtatics, page 61, 1ſt. Edit.

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‡ It is neceſſary to obſerve that though D. C. gives *two* Experiments under this Propoſition, they are in reality but different parts of the ſame Experiment; the one aſcertaining the heat of arterial, the other that of venous blood,—neither of which ſeparately determined is ſufficient to eſtabliſh the truth of the Propoſition.

* The proportion of heat in arterial and venous blood is given in this Experiment only as 2.3 to 2.

The third Propoſition, like the preceding one, ſeems to be ſupported by very few Experiments; and theſe being only on *one* claſs of bodies muſt render a general concluſion deduced from them reſpecting *all* bodies exceedingly precarious. To ſay that every ſubſtance in nature has leſs heat in proportion as its phlogiſton is increaſed, merely becauſe *metals* may have this property, is to take a vaſt deal too much for granted, and to betray a greater fondneſs for ſyſtems than appears to be conſiſtent with true philoſophy.—Let it be admitted that Dr. C. has proved to a certainty that tin, lead, iron, and antimony contain leſs heat than their calces; and let it be further admitted that all metals have their heat diminiſhed as their phlogiſton is increaſed: Still if the following Experiments, made upon the ſame principles, demonſtrate this *not* to be the caſe with reſpect to all the different bodies either in the mineral or vegetable kingdom, it muſt alſo be admitted that Dr. C.'s ſyſtem is not founded upon an *univerſal* law of nature; and that it is peculiarly unfit for explaining the phænomena of combuſtion, which are known to be moſt powerful in thoſe very ſubſtances which prove to be exceptions to his general rule.

EXPERIMENT IV.

Air in the room being 56, a quarter of a pound of dry ſaw duſt * at 58 was mixed with one pound and a half of water at 172, the mixture at the end of

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* The reaſon for taking thoſe ſubſtances in ſuch ſmall quantities is their being ſo exceedingly light, that the water

	Surface	Bottom	Medium
the firſt minute	162½	154	158¼
the ſecond minute	160	150	155
the third minute	157½	146½	152

Adding 3.19 for the heat carried off the firſt minute we have 161.44 for the true temperature of the mixture. The capacity of the veſſel for containing heat to that of water was as 1 to 12, and ſince this veſſel was raiſed from 58 to 158¼ it follows that it carried off 8.35 degrees of heat. The water was cooled 10.56 by the ſaw-duſt and veſſel together. It was cooled therefore 2.21 degrees by the ſaw-duſt alone. The ſaw-duſt was raiſed from 58 to 161.44, or 103.44 degrees. Conſequently the heat of a given quantity of ſaw-duſt to ſix times that quantity of water is as 2.21 to 103.44; and the heat of equal quantities of thoſe bodies is in the ratio of 13.26 to 103.44, or of 1 to 7.8.

water would not cover them in a larger proportion. This however, according to Dr. C. (*a*) makes no difference in the reſult of the Experiment.

(*a*) *See page* 12, *&c. See alſo Magellan's Eſſai ſur la nouvelle theorie du feu Elementaire &c. page* 168, 169, *&c.*

EXPERIMENT V.

Air in the room being 61, a quarter of a pound of pot aſhes at 65 was mixed with one pound and a half of water at $177\frac{1}{2}$. The mixture at the end of

	Surface	Bottom	Medium
the firſt minute was	167	165	166
the ſecond minute	161	$164\frac{1}{2}$	$162\frac{3}{4}$
the third minute	157	164	$160\frac{1}{2}$

From hence it may be found by purſuing the ſame ſteps with thoſe in the preceding Experiment, that the heats of equal quantities of water and calcined wood are in the ratio of 23.6 to 1. It follows therefore that the abſolute heat of dried wood is to that of calcined wood in the ratio of 23.6 to 7.8 or of 3 to one.

EXPERIMENT VI.

Air in the room being 42, half a pound of dry pit coal at 42 was mixed with one pound of water at 167. The mixture at the end of

	Surface	Bottom	Medium
the firſt minute was	144	140	142
the ſecond minute	142	140	141
the third minute	139	$139\frac{1}{2}$	$139\frac{1}{4}$

The capacity of the veſſel for containing heat was as above. From theſe *data* the abſolute heats of equal quantities of coal and water may be found to be in the ratio of 1 to 3.4.

EXPERIMENT VII.

Air in the room being 42, half a pound of coal perfectly calcined at 43 was mixed with one pound of water at 150. The mixture at the end of

	Surface	Bottom	Medium.
the firſt minute was	132$\frac{1}{2}$	131$\frac{1}{2}$	132
the ſecond minute	131	131$\frac{1}{}$	131$\frac{1}{4}$
the third minute	129	131	130

From hence it will appear that the heat of calcined coal to that of an equal quantity of water is in the ratio of 1 to 5.—Conſequently, the abſolute heat of dried coal is to that of calcined coal in the ratio of 5 to 3.4.

In addition to theſe Experiments it will not be improper to obſerve that Mr. *Kirwan* determines the abſolute heat of alcohol to be in proportion to that of water as 108.6 to 100[*]

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[*] See Mr. *Magellan's* Eſſai ſur la nouvelle Theorie du Feu elementaire, &c. (page 177); where it is alſo aſſerted that a ſolution of brown ſugar and water contains more heat than water alone in the ſame proportion as alcohol does;

It is unneceſſary to comment on thoſe facts. If we reaſon from Dr. C.'s principles, it is evident that the concluſions to be deduced from them muſt be directly contrary to his ſyſtem of combuſtion.

SECTION the FOURTH.

On the Application of Dr. CRAWFORD'S *Theory to the phænomena of Heat, Combuſtion, &c.*

IT ſeems to appear from what is ſaid in the foregoing ſections, that Dr. Crawford's Experiments are far from eſtabliſhing his ſyſtem. And this, if true, renders it unneceſſary to make uſe of any further arguments. But let us ſuppoſe that all thoſe Experiments are accurate, that the quantity of abſolute heat in air is in proportion to its purity, and that the quantity of this heat in all bodies is diminiſhed as their phlogiſton

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does; that is, in the ratio of 108.6 to 100. Now it is well known that ſugar is a very inflammable body. Should it not therefore follow from Dr. C.'s theory, that this ſolution contained *leſs* abſolute heat than water?

is increaſed ;—ſtill, however, the ſyſtem which has been deduced from theſe *data* is liable to ſuch a variety of objections, and appears in ſo many inſtances to contradict the facts which have been recorded by other philoſophers, that I cannot help extending my enquiries to this part of the ſubject.

It may be obſerved that Dr. C. has taken much pains to determine the abſolute heat of fixed air ; and, admitting the principles and reſults of his Experiments to be juſt, it appears that the ratio of the heat of this kind of air is to that of atmoſpherical air as 1 to 67.—From hence he concludes * that the ſame heat which raiſes the latter any given number of degrees, will raiſe the former the ſame number of degrees multiplied by 67.—Proceeding on this ground he computes the heat which is yielded by atmoſpherical air when it is converted into fixed air in the following manner.—“ In the “ Peterſburgh Experiment the heat was di- “ miniſhed 200 degrees below the common “ temperature of the atmoſphere. We are “ therefore certain that atmoſpherical air when “ at the common temperature of the atmoſphere

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* Page 70.

“ contains at leaſt 200 degrees of heat. Hence if “ a certain quantity of atmoſpherical air not in “ contact with any body that would immedi- “ ately carry off the heat ſhould ſuddenly be “ converted into fixed air, the heat which was “ contained in the former would raiſe the latter “ 200° multiplied by 67 or 13,400 degrees:[*] “ And the heat of red hot iron being 1050, “ it follows that the quantity of heat which is “ yielded by atmoſpherical air when it is con- “ verted into fixed air is ſuch, (if it were not “ diſſipated) as would raiſe the air ſo changed to “ more than 12 times the heat of red hot iron.”[†] —By the aſſiſtance of this calculus and the third propoſition Dr. C. explains the whole myſtery of combuſtion. “ Inflammable bodies “ (he obſerves) abound with phlogiſton and “ contain little abſolute heat. Atmoſpherical

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[*] Does not this reaſoning ſuppoſe that fixed air has no heat in it; or that common air in combuſtion gives away its whole heat, leaving none for the fixed air into which it is reſolved?—If any is left in this fixed or rather phlogiſticated air, will not the quantity of fire produced be ſo much leſs than this ſtrange calculation makes it?—In truth the 200 degrees here mentioned make no addition to the fire produced: The reſult of the combuſtion being, according to Dr. C.'s ſyſtem, an air having only one degree of heat, inſtead of an air having 67 degrees.

[†] page. 70.

" air on the contrary abounds with abſolute
" heat and contains little phlogiſton. In the
" proceſs of inflammation the phlogiſton is
" ſeparated from the inflammable body and
" combined with the air. The air is converted
" into fixed and phlogiſticated air, and at the
" ſame time gives off a very great proportion
" of its abſolute heat, which when extricated
" ſuddenly burſts forth into flame, and pro-
" duces an intenſe degree of ſenſible heat.
" We have found by calculation that the heat
" which is produced by the converſion of at-
" moſpherical into fixed air is ſuch (if it were
" not diſſipated) as would be ſufficient to
" raiſe the air ſo changed to more than 12
" times the heat of red hot iron. It appears
" therefore that in the proceſs of inflammation
" a very great quantity of heat is derived from
" the air. It is manifeſt on the contrary that
" no part of the heat can be derived from
" the combuſtible body.*"

Such are the outlines of this wonderful theory.—A theory which, if it be true, well deſerves the epithet of *brilliant*, and every encomium which its moſt zealous admirers can beſtow upon it.—But I cannot help at preſent conſidering it as rather more *brilliant* than *ſolid*; and the following obſervations may perhaps in ſome meaſure juſtify this opinion.

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* Page 76.

That the ſame heat which raiſes common air one degree ſhould, as Dr. C. aſſerts, raiſe fixed air 67 degrees, appears to be a fact ſo extraordinary, and at the ſame time, if true, ſo eaſily demonſtrated, that I am ſurprized we have no other proof of its exiſtence than a mere deduction from his very complicated Experiments on chalk and quicklime.—If heat produces effects ſo immenſely greater on fixed than it does on common air; then a thermometer in ſimiliar circumſtances would riſe many times faſter in the one when heated than in the other:—And yet I have never found the leaſt difference.

EXPERIMENT.

Having filled two ſeparate bladders, the one containing a pint of fixed air, and the other an equal quantity of common air at 75°;—and having fixed a thermometer in each of thoſe bladders as in fig. 1ſt and tied to their reſpective bottoms a large piece of lead in order to make them ſink more readily, I took a bladder in each hand and plunged them both at the ſame inſtant into a pail of water at 150°, when it appeared that the mercury roſe as faſt in the fixed as it did in the common air. *

In the ſame manner I tried common and inflammable airs, and alſo phlogiſticated and dephlogiſticated airs, (in which caſe the one ought to have heated 300 times faſter than the other,) but the reſults of all thoſe Experiments were much the ſame.—Nay, Sir *Iſaac Newton* obſerves that " a thermometer even *in vacuo*

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* Should it be urged, that Dr. C. only expreſſed himſelf incorrectly, in ſaying that the ſame heat which raiſes common air 1 degree will raiſe fixed air 67 degrees ; and that his meaning is only that common air in a given degree of ſenſible heat contains in a latent ſtate 67 times as much heat as fixed air of the ſame degree of ſenſible heat ; yet if by the the degree of *abſolute* heat be meant, what alone it properly ſignifies, *the different capacities of being heated and cooled*; or *different ſuſceptibilities of the action of fire*, then does this experiment undeniably prove that there is no difference in the degree of this abſolute heat in fixed and common air. Were there any ſuch difference the thermometer would riſe faſter in the one than in the other, as is known to be the caſe with regard to quickſilver and water, metals and their calces, &c.

" will grow warm as much and almoſt as ſoon " as a thermometer which is not *in vacuo*." † How is it poſſible therefore that the effects ſhould be ſo different in the inſtances which Dr. C. mentions, while they are ſo nearly the ſame in this caſe, where one thermometer is placed in a medium containing, according to him, an immenſe quantity of heat, and another is placed where no heat can be ſuppoſed to exiſt ?

In the Peterſburgh Experiment, it is ſaid, " that the heat was diminiſhed 200 degrees " below the common temperature of the at- " moſphere, and therefore we are certain that " atmoſpherical air when at the common tem- " perature of the atmoſphere contains at leaſt " 200 degrees of heat."—I muſt confeſs that I do not underſtand this language. It ſeems to me as intelligible as if it were ſaid, that a loaded mortar whoſe amplitude is two miles contains two miles of force. But the ſubſequent calculation is as inconceivable as this language is unintelligible. When we are told that " atmoſpherical air " by being ſuddenly converted into fixed air is " capable of raiſing the latter 13,400 degrees,

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† Newton's Optics, quæ. 18.

"or to 12 times the heat of red hot iron," we are indeed *aſtoniſhed*; but I do not think we can be much *edified.* * Dr. C. has not proved that any ſuch converſion ever takes place; and it is particularly worthy notice here, that he has confounded phlogiſticated and fixed air with one another; for his whole reaſoning on the heat of combuſtion depends on the opinion that fixed air is a combination of common air and phlogiſton.—An opinion ſo diametrically oppoſite to the conſtant tenor of Dr. *Prieſtley's* Experiments, that this excellent philoſopher even complains of Mr. *Lavoiſier* for having repreſented him as maintaining ſuch an error. || He obſerves alſo on another occaſion that "many perſons have confounded phlogiſticated "with fixed air, having concluded the whole "of a quantity of air to be of the latter "kind, though by far the greateſt part of it "was of the former. †"—In the calcination of metals

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* See note in page 53 of this treatiſe.

|| See Dr. *Prieſtley's* Experiments on air, vol. 2. page 313.—The reader may alſo conſult the Experiments of the truly ingenious Mr. *Bewly*, which are inſerted at the end of this volume, and which evidently prove fixed air to be an *acid ſui generis*.

† Dr. Prieſtley's Experiments on air vol. 4, page 308.

See

metals we are informed § that "the phlogiſton "which is ſeparated from them is combined "with the air, and that this combination has "been proved to produce a very intenſe heat." The manner in which this hath been proved leaves no room to doubt but that Dr. C. conſiders fixed air as a compound of phlogiſton and common air; and alſo the intenſe heat he mentions, as the conſequence of fixed air's being ſo eaſily affected by the fire which is diſengaged from the atmoſphere during combuſtion, —That fixed air, however, is not accumulated in any quantity round metals when calcined, and of courſe that it cannot be the cauſe of the heat attending their fuſion appears evidently from an Experiment of Dr. *Prieſtley*, by which it is proved that "lime-"water never became turbid by the calcination "of metals over it, but that the calx immedi-"ately ſeized the precipitated fixed air in pre-"ference to the lime in the water."* Beſides, if common air is converted into fixed air by the

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See likewiſe his letter to Mr. Kirwan at the end of Scheele's Experiments on air and fire, tranſlated by Dr. Forſter.

§ Dr. C.'s treatiſe, page 76,

* Dr. Prieſtley's Experiments on air, vol. 1, page 137.

inflam-

inflammation of bodies, and this inflammation continues while there is any common air to be decompoſed by the phlogiſton which is ſeparated from thoſe bodies, then it will follow from the known property of fixed air that the whole of a given quantity of common air in which a candle has burned out will be abſorbed by water.—But nothing like this ever happens. —Seeing therefore that fixed air is neither heated faſter than atmoſpherical air, nor generated in any conſiderable quantity during the combuſtion of bodies, we may ſafely conclude, that it cannot be the cauſe of that ſenſible heat which attends this combuſtion.—And ſince Dr. C. hath aſſigned no other cauſe of that heat, nor even attempted to aſcertain the heat of *phlogiſticated* air; I think it muſt be acknowledged that his theory, ſo far as it is ſupported by his preſent Experiments, is very defective, and communicates but little real information on this obſcure ſubject.

With regard to Dr. C.'s *mathematical* propoſitions, I have only to obſerve, that allowing the *data* to be right, I believe the demonſtrations are univerſally true. But for my own part, I ſhould not wiſh to introduce the mathematics on ſuch occaſions, being ſatisfied that ſubjects like the preſent ſtand upon too precarious grounds to admit of their uſe.—Nor can I ſee any ſolution given by Dr. C. which could not eaſily have been obtained by plain reaſoning, without the aſſiſtance of algebraical ſigns and ſymbols.*

Dr. C. obſerves that if his doctrine be true, it affords an eaſy ſolution to the moſt remarkable facts relating to animal heat and combuſtion. * We have already ſeen how well it accounts for combuſtion in general, and it may not perhaps be improper to ſee, whether it accounts for thoſe remarkable facts with better ſucceſs.

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* It is ſaid in Cor. 1, Prop. 3. "that in order that "neither heat nor cold ſhould be produced by the paſſage "of phlogiſton from one body to another, the differences "of the abſolute heat ſhould be equal, or that $H-h$ "ſhould be equal to $P-p$."—Hence we have the following corollaries, which will not perhaps be conſidered by a mathematical reader as very important.

Cor. 4. "If h be equal to p, in order that neither heat "nor cold ſhould be produced, it is required that P ſhould "be equal to H; for in that caſe $H-h=P-p$."

Cor. 5, "The ſame things being ſuppoſed, if h be "leſs than p, then P muſt be greater than H, and P will be equal to $H+p-h$, for in that caſe $H-h=P-p$"

Cor. 6. "If h be greater than p, P muſt be leſs than H, "and P will be equal to $H-\overline{h-p}$, for ſubtracting $h-p$ "from H, we have $H-h+p=P$, and therefore $P-p=$ "$H-h$."

It does not ſeem very eaſy to perceive what difference takes place in thoſe two laſt corollaries, in conſequence of h being leſs or greater than p; for in the one caſe P is equal to $H+p-h$, and in the other P is equal to $H-\overline{h-p}=H+p-h$; that is P is equal to P.

The firſt of this kind is the ſpark of fire produced by the percuſſion of flint and ſteel, which our author explains in the following manner: "During the inflammation of iron "a quantity of heat becomes redundant which "would be ſufficient to raiſe the calx 11,480 "degrees. Hence when a particle of this metal "is ſtruck off by the force of a flint, the phlogiſton is ſeparated from the particle, and is "left at liberty to combine with the air; in "conſequence of which a quantity of fire is "diſengaged from the latter, and the heat "which is produced during this proceſs is ſo "intenſe, that the particle of the metal which "is ſtruck off is converted into glaſs." † On this ſuppoſition then it is neceſſary that the phlogiſton ſhould be diſengaged *previous* to the decompoſition of the air, and conſequently the metal is calcined and even vitrified before any heat is produced. There are no experiments which prove that the percuſſion of ſteel by flint can cauſe ſuch a decompoſition in the former as to force away its phlogiſton; nor has Dr. C. ever given us the leaſt intimation of this extraordinary phænomenon before he aſſumes it in the preſent ſolution.—Beſides, if the intenſe heat produced by the percuſſion of two bodies be owing alſo to ſuch a decompoſition of the *air* as he mentions; how comes it to paſs that heat is produced by the attrition of two bodies *in vacuo*?* Or rather, how comes it to paſs that this intenſe heat, this very vitrification of the metal is produced by the colliſion of flint and ſteel in an *exhauſter receiver* ‡.

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* Page 79.

† Page 108.

Another phænomenon explained by Dr. C. is the accenſion of eſſential oils when mixed with the nitrous acid. In this caſe it is ſaid, ‖ "the

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* Adeoq; in ipſo quoq; *vacuo* attritu mutuo corpora incaleſcunt; uti obſervationibus accuratiſſimis egregii Haukſbei aperte conſtat, quæ & more ſuo fœliciſſimè promovit vir clariſſimus, conjunctiſſimus collega, Jacobus Gulielmus Graveſande. Vide Boerhaav. de Igne, Exper. 9. Cor. 3.

‡ See the 4th of Mr. *Boyle's* Phyſico-mechanical Experiments touching the ſpring of the air.—And the 8th of of his Experiments touching the relation between flame and air.

‖ Page 110.

"phlogiſton is ſeparated from the oil and combined with the acid: The latter is forced to "give out a portion of its abſolute heat, part of "which is abſorbed by the baſis of the oil "and the reſt becomes redundant or is converted into ſenſible heat. If the ſenſible "heat be increaſed to a certain degree the phlogiſton will ſuddenly combine with the air, "in conſequence of which a great quantity of "fire will be extricated, and the whole will "explode with intenſe heat."—Had Dr. C. known that the mixture is capable of being inflamed with immenſe violence *in vacuo*, * he would probably have ſpared himſelf the trouble of this laboured explanation.

A third phænomenon which is ſuppoſed to admit of a ſolution by Dr. C.'s theory is the heat imparted to the air by the *electric ſpark*. Here it is obſerved that "ſince Dr. Prieſtley has "proved the electric fluid to be capable of "communicating phlogiſton to atmoſpherical "air and of converting it into fixed and phlogiſticated air, it muſt follow that the heat

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* See Dr. *Slare's* chemical Experiments in the 3d vol. of the Philoſophical Tranſactions, abridged by Lowthorpe, page 364.

" which becomes ſenſible on this occaſion is diſ-
" engaged from the atmoſpherical air in conſe-
" quence of the phlogiſton with which it is im-
" pregnated." * That the electric ſpark diminiſhes common air, and that it alſo raiſes the mercury in the thermometer, are both very true; but on the other hand, it is equally true that the ſame effect is produced upon the thermometer, when the ſpark is even drawn thro' a *Toricellian vacuum* ‡, and therefore it is evident that the ſenſible heat it cauſes can by no means depend on the decompoſition of atmoſpherical air.

The heat, noiſe, commotion, and all the dreadful phænomena of *earthquakes*, Dr. C. ſeems to think will admit of an eaſy ſolution by his theory.—He begins with obſerving, that they may all be imitated by burying under ground a mixture of iron filings and brimſtone made into a paſte with water †; and he propoſes the following explanation of thoſe artificial earthquakes as the means of enabling us to account for the real ones.—“ The attraction “ of the phlogiſton to the acid of the ſulphur “ will be diminiſhed both by the attraction of the “ iron to this acid, and by that of the water. “ In the degree of heat which is neceſſary to the “ inflammation of ſulphur, atmoſpherical air “ is capable of ſeparating the phlogiſton from “ the vitriolic acid. Is it not probable that by “ the aſſiſtance of the iron and the water, it “ may be capable of producing this effect in the “ common temperature of the atmoſphere. If “ this be the caſe, it follows, that by the “ action of the air, which is diffuſed through “ the ſubſtance of the earth, upon the phlogiſton of the ſulphur, and by that of the iron “ and water upon the acid, the ſulphur will be “ decompoſed; the air will unite with the “ phlogiſton; the iron with the acid; a quantity of fire will be diſcharged from the former, “ and an inflammable elaſtic fluid from the “ latter; and hence a commotion will be excited, accompanied with noiſe and the eruption “ of flame, reſembling the phænomena of an “ earthquake.”*

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* Page 111.

‡ The ſpark was drawn from a very large conductor thro' a *vacuum* one inch long, and $\frac{3}{8}$ of an inch in diameter.—the mercury roſe in leſs than a minute two degrees; nor did I find it riſe much, if at all, faſter when made to paſs through common air exactly in the ſame circumſtances.

† Page 112.

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* Page 112.

I

I do not think that the decompoſitions here mentioned are ſtrictly conſonant with the laws of the elective attractions of bodies.—The affinity of acids to phlogiſton has been generally underſtood to be much ſtronger than it is to metals of any kind. But Dr. C. by ſuppoſing the iron in the preſent inſtance to decompoſe the ſulphur, ſeems to be of a contrary opinion.—Admitting this to be the caſe; may it not be doubted whether the air, which is ſaid to be diffuſed through the ſubſtance of the earth, is the cauſe of thoſe phænomena; eſpecially conſidering that ſo very ſmall a portion of it can be ſuppoſed to come in contact with the mixture? How comes it to paſs, if the noiſe and commotion depend on the decompoſition of the air, that the ſame effects are not produced when this mixture is placed in a large jar of air having its mouth immerſed in water? Might it not be expected on Dr. C.'s principles, that the air in thoſe circumſtances would be greatly expanded, and in conſequence force the water out of the jar with conſiderable violence? We know however that no ſuch effects are produced; and that though the air is heated, it is ſo far from being expanded by the iron filings and ſulphur, that it is diminiſhed a *fourth*,* or

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*See Dr. Prieſtley's Experiments on air, vol. 1. ſect. 6.

even a *third* * in its original bulk. Nor is the heat produced by the mixture of theſe two ſubſtances by any means confined to common air. Dr. *Prieſtley* found that when they were placed in nitrous air (which he ſuppoſes to contain more phlogiſton than inflammable air) † the jar which covered them became ſo hot that he could not even touch it. || Beſides, if the decompoſition of atmoſperical air by phlogiſton is always attended with ſuch an intenſe heat as to be raiſed 13,400 degrees, might we not reaſonably expect that the air, when it is phlogiſticated in a jar by this mixture, would become vaſtly hotter than we really find it to be. But perhaps it may be objected, that the phlogiſtication in this proceſs is ſo ſlow, that the heat is gradually diſſipated, and of conſequence its effects are rendered ſo much the leſs ſenſible. This, however, cannot be the caſe when nitrous and common air are mixed together. Here the latter is immediately phlogiſticated ; and yet I have never found the thermometer to be raiſed above ten degrees. What becomes of the re-

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* See Mr. Scheele's Experiments on air and fire tranſlated by Dr. Forſter, page 65.

† See Dr. Prieſtley's Experiments on air, vol. 3, ſect. 16.

|| See Dr. Prieſtley's Experiments on air, vol. 1, ſect. 6.

maining 13,390 degrees I know not: For neither the water ‡ nor the veſſel ſeem to have imbibed any part of it, if we are allowed to judge by the thermometer or our ſenſes.

In applying the ſolution of thoſe artificial earthquakes to that of the real ones, Dr. C. ſuppoſes a ſimilar mixture of ſulphureous and metallic bodies to be produced in the bowels of the earth, and that thoſe bodies, in conſequence of ſome changes which take place there, are brought into contact with the water and atmoſpherical air which are diffuſed through the earth's ſubſtance, or lodged in cavities beneath its ſurface.* But ſince Dr. C.'s ſolution of the phænomena attending artificial earthquakes labours under ſo many difficulties, and the mixtures which he ſuppoſes to happen in the real ones are merely conjectural, I ſhall beg leave to decline the further conſideration of this ſubject.

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‡ In making this Experiment I have placed a ſmall baſon under the mouth of the jar after mixing the nitrous and common air, and inſtantly taken the whole out of the tub: By which means if any heat were communicated to the water, it would be eaſily determined from the ſmall quantity in which the mouth of the jar is now immerſed.—The jar in which this Experiment was made contained about 3 ounce meaſures of air. Had a larger been uſed, it is probable the heat would have been more conſiderable. But this by no means removes the objection to Dr. C.'s ſyſtem. For (independent of the ſmall quantity of air which muſt be ſuppoſed to be phlogiſticated, according to his hypotheſis, in the colliſion of flint and ſteel, where ſuch powerful effects are produced) even if the thermometer were raiſed 100°, it would ſtill be difficult to account for the remaining 13,300° which are ſaid to be diſſipated by this phlogiſtication of the air.

Upon the whole, I wiſh Dr. Crawford had employed his abilities in making a greater number of Experiments, and in a more thorough inveſtigation of his theory before he propoſed it. He ſhould conſider, that this is an age that is too ready to adopt new theories, and that much harm is done by ſuch theories when wrong, by putting us back in the purſuit of truth, and occaſioning a ſad waſte of that time and attention of philoſophers in clearing away rubbiſh, which ſhould be employed in raiſing and improving the ſuperſtructure of knowledge. I will only add that I reſpect his character, and that I am perſuaded he means nothing but the advancement of ſcience; and that ſhould he, contrary to my expectations, by any new evidence remove my objections or eſtabliſh his theory, I ſhall thank him, and retract any miſtakes into which I may have fallen.

* Dr. C.'s Treatiſe, page 113.

FINIS.

Fig. 1.

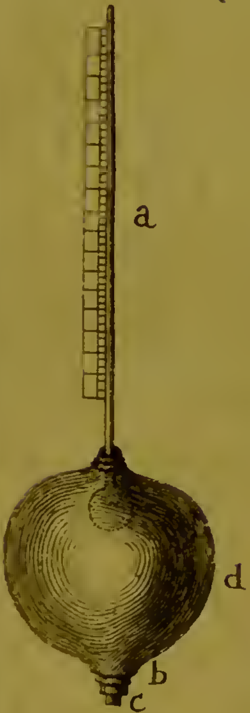
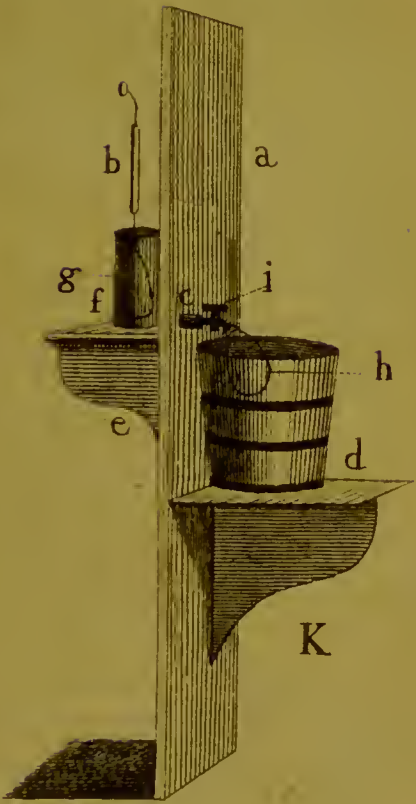


Fig. 2.



ERRATA.

Page 18, laſt line of the note, *for* $2\frac{1}{2}$ *read* $2\frac{1}{3}$. Page 22, line 14, *for* 3+14, *read* 3×14,—line 23, *for* to that, *read* to be to that. Page 28, line 11, *for* the, *read* its. Page 51, line 8, *for* to the phænomena, *read* to the ſolution of the phænomena.

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